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plateau. When these features are carefully examined with this "landslip" idea present in the mind, the evidences of such being the true cause and nature of their appearance become irresistible; inasmuch as that any one who has seen "landslips," and the fallen-in interior walls of volcanic craters, will be struck with the similarity of the aspect of the features in question.

With respect to the cause and the nature of the action which appears to me to have occasioned the features in question, I consider that the most probable explanation is, that during the most active state of those volcanic craters the matter ejected from the volcano accumulated, and was piled up to such a height on the summit of the great circular wall or crater, as by its crushing-down action, aided by the undermining and softening action of the intensely hot lake of molten lava (then occupying the bottom of the crater), the vast superincumbent mass forming the wall of the crater had, from time to time, and to a greater or less segmental extent, crushed and slid down, and so settled in a more permanent position: when occurring to a great segment, resulting in the "terrace-like" appearance; and when in a more limited extent yielding all the well-known features of a "*landslip*."

It occurs to me, that if any one (having suitable telescopic means) will carefully examine the details of these features, they will not fail to regard these remarks as worthy of consideration, as yielding what appears to me a rational explanation of their nature and origin.

I may state that I consider the era of the active condition of the lunar volcanoes to have gone by ages ago.

Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated January 17, 1854.[*]

"In the last months I have been so much engaged with other things that I could not find leisure to calculate my observations on the parallax of 61 *Cygni*. But Prof. Woldstedt being here on a visit in the month of September, has made a preliminary calculation, which gives

By the distances, the parallax $= +0''.542 \pm 0''.033$

By the positions, $= +0.504 \pm 0.045$

or, in the mean, a parallax greater by nearly one-third than Bessel's value. The proper motion deduced from my observations agrees within one or two hundredths of a second with that deduced from meridian observations. It was a very curious fact that, when first Woldstedt, by application of proper motion, had reduced the measures to one epoch, I was able to point out, by a single view, the epochs of the different maxima and minima, so exactly that the afterward calculated real epochs did in no case differ from my estimate by more than ten days. I do not regard Prof. Woldstedt's

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[*] For a further extract from the same letter, see p. 167.

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calculation as definitive, but the ultimate corrections to be applied to the values he has found, I suppose, will not exceed 0″·02. How to explain the difference between Bessel's determination and mine is more than I can say at present. Perhaps it has originated merely from an accumulation of accidental error in opposite directions for the two series; but, more probably, it might be attributed in part to the action of the same uncommon causes, which evidently have disturbed in much higher degrees Wichmann's observations, and about which Doellen's researches contain very curious indications.

"In speaking here of constant or systematical errors, to which heliometrical measures are subject, it would not be right to avoid mentioning that likewise my own measures, made with the wire-micrometer, are not free from similar errors. On the contrary, a long series of experiments, made on artificial double stars, indicate very important corrections to be applied to all my measures of *close* double stars. From a great number of observations I have deduced the following empirical formula for the correction of positions, measured by me on double stars, 0″·8 to 6″ of distance:—

$$\text{Corr.} = + \frac{4^{\circ}{\cdot}23}{\alpha} + 3^{\circ}{\cdot}24 \sin(2\,\varphi - 29^{\circ}\,11') + 1^{\circ}{\cdot}51 \sin(4\,\varphi - 31^{\circ}\,29')$$

"In this formula α is the distance expressed in seconds and φ the angle between the true direction of the double star and the vertical circle passing through the star at the time of observation.

"At about 6″ of distance the method of observation changes. It must be attributed to this circumstance, that for greater distances the coefficients of the empirical formulæ diminish rapidly, so that, at a distance of 12″, the systematical errors disappear altogether, or are incomparably smaller than the little accidental errors of observation. Hence it follows that my determination of parallaxes is quite free from these systematical errors.

"The constant errors of the measured distances are, generally speaking, less important, as it might be seen from the following list:—

True Dist.	Corr. of Meas. Dist.	True Dist.	Corr. of Meas. Dist.
″	″	″	″
0·99	+ 0·029	7·88	+ 0·094
1·94	0·079	11·60	+ 0·032
3·88	+ 0·136		

"I think the origin of all these errors in the measured positions is quite physiological, and it is highly probable that all practical observers will be subject to similar systematic errors. It requires no demonstration, that similar corrections will change *very considerably* all hitherto deduced elements of the orbits of double stars, especially as these are commonly deduced from the observed positions. Therefore, in future times, no other observations ought to be employed for that purpose, but those for which the personal corrections are well ascertained by direct experiments or by comparison with corrected observations.

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" One other consequence which might be deduced from the existence of such errors is, that we must be very sceptical with regard to parallaxes deduced in the way in which it had been done by Capt. Jacob for *α Herculis;* most probably the parallax he has found in this case is nothing more than a strong indication that the positions observed by him are subject to similar systematic errors as mine: for in itself it is highly improbable that *α Herculis* and its companion should have different parallaxes. My researches on the constant errors of my observations are not yet closed, for there are yet many questions undecided. I shall continue them most probably throughout the whole ensuing summer, and only when they are finished I shall proceed to the publication of my fifteen years' observations of double stars."

On Two New Theorems in Lunar Astronomy. By Professor Challis.

(*Extract of a Letter from Professor Challis to the Editor.*)

" I have succeeded in demonstrating, on the principle of gravitation, two new theorems relating to the moon's orbit.

" Theorem I. All small quantities of the second order being taken into account, the relation between the radius vector and the time in the moon's orbit is the same as that in an orbit described by a body acted upon by a force tending to a fixed centre.

" If μ be the sum of the attractions of the earth and moon, and m' the attraction of the sun at the unit of distance, and if r be the moon's radius vector, and a' the sun's mean distance from the earth, the central force is $\frac{\mu}{r^2} - \frac{m'r}{2a'^3}$. The theorem is true inclusive of the moon's motion in latitude.

" The integration by which the relation between the radius vector and the true longitude on the orbit is obtained, gives directly the mean motion of the apse to the first approximation. The same integration involves the following consequence:—

" Theorem II. The excentricity of the moon's orbit is a function of the ratio of the moon's periodic time to the earth's periodic time, and, to the first approximation, is equal to that ratio divided by the square root of 2.

" This theorem applies to the *constant* part of the excentricity, and depends entirely on the circumstance that the apse has a mean motion.

" The proofs of these theorems are contained in a communication to the Cambridge Philosophical Society.

" *Cambridge Observatory, March* 9, 1854."

An Account of the Hartwell Rectory Observatory.

By the Rev. C. Lowndes.*

" It might be thought that an Observatory on the Rectory grounds at Hartwell would be quite unnecessary, as Dr. Lee has

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* A model of this observatory has been sent to the apartments of the Society, which Dr. Lee explained at the last Meeting.